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PRELIMINARY NOTES ON THE HISTORICAL SIGNIFICANCE OF QUANTIFICATION AND OF THE AXIOMS OF CHOICE IN THE DEVELOPMENT OF MATHEMATICAL ANALYSIS

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This paper falls into three parts. The first two deal respectively with quantification and the axioms of choice in real-variable mathematical analysis, while the third discusses historical significance in general. This leaves me to explain the words "preliminary notes" of my title. They indicate that each part is intended only to call attention to certain aspects of its subject matter which have not been studied to the extent that they deserve. Many details are omitted, and some over-simplification inevitably results. However, it is hoped that the points made will provide sufficient compensation.

PART 1: ON THE EMERGENCE OF QUANTIFICATION, AS EXEMPLIFIED IN MODES OF CONVERGENCE.

I use the term "quantification" in mathematical analysis to refer to the techniques of multiple limits, where increments are taken on variables denoting real numbers and values chosen for suffix variables from their ranges (usually the integers), and the functional relationships between these variables are represented by an explicit statement of quantifier order. The language used is at least partly symbolic, though not necessarily the full apparatus of the predicate calculus. A typical symbolic expression in such analysis might begin, say, with

$$(A\varepsilon)\,(E\delta)\,(EN)\,(Ax)\,[\varepsilon>0.0<|x-x_0|<\delta\ldots$$

I shall exemplify the emergence of quantification by describing the history of the correction of a well-known false theorem. It comes from Cauchy's *Cours d'analyse* of 1821 [a, 120] and reads as follows: "When the different terms of the series [Σu_i] are functions of the same variable x, continuous with respect to that variable in the vicinity of a particular value for which the series is convergent, the sum s of the series is also a continuous function of x in the vicinity of that particular value."

Abel seems to have been the first to raise public objection to this theorem, when he pointed out in 1826 that the Fourier sine series for $x/2$ over $[0,\pi]$, with its discontinuity at $x=(2m+1)\pi$ for all integers m, was a counter-example [b, thm.V.]. In terms of multiple limits, he was surely thinking of the behaviour of the iterated limited of the n-th partial sum of the series as $x \to a$ and then $n \to \infty$. But multiple limits were not handled then with the precision to which we are now accustomed, nor were the underlying issues understood. For example, nobody at the time seems to have suggested a possible reconciliation between Cauchy's theorem and the counter example by interpreting both as concerned with the *double* limit of the partial sum as $n \to \infty$ and $x \to a$. Yet this in effect is how Fourier himself conceived his series (though to the modern view his treatment was unrigorous); for in both his diagrams and his verbal descriptions of his series he included vertical lines across the discontinuities (See my and Ravetz's *Fourier* [B13], pp. 158-170; 192-193; 220-227). He seems to have conceived a series as the geometrically connected--and thus continuous--limiting curve of its continuous partial sum curves; and if Cauchy's theorem could be applied to such limiting curves, then a reconciliation might be effected. But this development does not seem to have been tried, though a first step may be seen in Dirksen in his 1829 review [c] of Cauchy's *Cours* (See my [B13], p. 116).

However, there *was* a tendency in the 1820s (not to say later) to think of continuous functions as "safe" and so to confine analysis to them. Cauchy's theorem exemplifies the tendency in asserting the continuity of the sum-function. So does Abel's work, for although he pointed out the (apparent) counter example, he contributed to "safe" theory by proving his famous limit theorems. Indeed, he mentioned the counter example in a footnote to a theorem which relates to Cauchy's: if $u_n(x)$ is continuous over an interval in x and $\Sigma u_i(x)\delta^i$ converges, then when $(0<)\alpha<\delta$, $\Sigma u_i(x)\alpha^i$ is convergent, and continuous in α up to δ [b, thm.V.].

The unreserved acceptance of discontinuous functions into analysis was first shown in 1829 [B9] by Dirichlet with his sufficient conditions for the convergence of Fourier series, where a finite number of finite discontinuities were admitted. In terms of multiple limits, he started with the iterated limit where $x \to a$ and then $n \to \infty$ and preserved it throughout his proof (See my 1970 *The Development of the Foundations of Mathematical Analysis*, pp. 97-104). Perhaps significantly, it was his student Seidel who was to make the first significant modification to Cauchy's theorem. But before we study his work, let us look at Cauchy's own proof to see how it fails.

Cauchy's proof hinges on the equation

$$(1) \qquad s(x+\alpha)-s(x) = (s_n(x+\alpha)-s_n(x))+(r_n(x+\alpha)-r_n(x)),$$

where $s_n(x)$ is continuous, and $r_n(x)$ is small. But Cauchy went further to assert that $(r_n(x+\alpha)-r_n(x))$ "becomes insensible at the same time" as $r_n(x)$, thus proving the theorem [a, p. 120]. Now to the modern view it is obvious that uniform convergence is tacitly assumed here. But historically it is not easy to say *how* it is being assumed, or which mode is being invoked. We need to examine his phrase "in the vicinity" from his statement of the theorem, referring to the neighbourhood of the value of x at which convergence takes place, together with his expression "becomes insensible at the same time" in his proof, describing the mutual smallness of $r_n(x+\alpha)-r_n(x)$ and $r_n(x)$. Is the magnitude of the vicinity dependent on that of the insensibility (when we have something like uniform convergence at a point), or is it independent (which results in a version of uniform convergence in the neighbourhood of the point)? Or was Cauchy perhaps thinking of uniform convergence over an interval, in line with his definition of the continuity of a function which, though formulated in terms of local behaviour, defines the property at all points of an interval [a, pp. 34-35]? Surely the answer to these questions is that Cauchy had *none* of these modes of uniform convergence consciously in mind, and that his proof is intrinsically vague with respect to them. This interpretation is strengthened by the fact that, although his proof hinged on (1), he did not write it down; in fact, he wrote only "u_n", "s_n", and "r_n", with "x" omitted, for the various parts of the series. In other words, his symbolic language was inadequate in a crucial respect.

Now let us consider Seidel's modification, made in a paper [B37] of 1848. He began by remarking on the contradiction between Cauchy's theorem and Dirichlet's conditions, and proposed the following definition: the series $\sum u_r(x)$ (and Seidel *did* include the argument variable in his symbolism, actually writing "$F(x,n)$") converged "not arbitrarily slowly" if ν, the value of n for which $|r_n(x+\varepsilon)| < \rho$ for all $n > \nu$, tends to infinity as ε tends to zero. By laborious means (described in [B13, pp. 112-114]), he vindicated Cauchy's theorem for not arbitrarily slow convergence. But it would be historically naive to *identify* Seidel's definition with uniform convergence, for the formulation is again not in that style, although it differs also from Cauchy's phraseology in the *Cours*. Although ν depends on ε, their relationship to ρ is not clear. More importantly, in requiring ε to *achieve* its limiting value zero, Seidel spoiled the limit-avoiding style of mathematical analysis that he was trying to enrich. Similar comments can be made about a definition of "not infinitely slow" convergence which Stokes introduced independently around the same time [B38]. In that case, quasi-uniform convergence in the neighbourhood of a point seems to be the closest approximation [B13, pp. 113-117]. It is interesting to note that at exactly the time that Seidel and Stokes were refining Cauchy's theorem, Wilbraham was modifying Fourier's geometrical representation of Fourier series and introducing, in his 1848 paper [d], the mis-named "Gibbs phenomenon."

The modification of Cauchy's theorem to include uniform convergence as we understand it was made by Cauchy himself in a paper [B7] of 1853. He expressed uniformity by suitably modifying his necessary and sufficient condition for convergence: $|s_{n'}(x)-s_n(x)|$ "becomes always infinitely small for infinitely large values of the numbers n and $n'>n$." The context made clear that "always" referred to all values of x over the interval in question, so that uniform convergence over that interval was now embodied in the conditions of the theorem. *Now* the functional relationship between the increments could be clearly understood, and the exegesis of multiple-limit analysis carried out properly.

However, I do not wish to imply that it is to Cauchy that we owe the development of modes of convergence. His paper, a rather hastily written piece, has little in it for real-variable analysis beyond the modified theorem and necessary and sufficient condition. The new leader was Weierstrass, who used the term "uniform convergence" in a manuscript of 1841 [B39, vol. 1, pp. 68-69]--that is, before Seidel and Stokes. He may have taken the idea (though not the terminology) from his teacher Gudermann [B10, p. 47].

The history of the development of Weierstrassian multiple-limit analysis is difficult to describe, for much of the inspiration came from Weierstrass's unpublished lectures at Berlin. The first published manifestation of the new ideas came from others. For example, in 1870, Heine remarked that Weierstrass had noticed the assumption of uniform convergence over an interval in the proof that a series of functions can be integrated term-by-term, and he himself introduced uniform continuity [B20].

Over the rest of the century and beyond other modes of uniform, quasi-uniform and non-(quasi-) uniform convergence were gradually introduced and the relationships between them explored [B19]. An important example is Weierstrass's proof in 1880 [x, art. 1= B39, vol. 2, pp. 201-233] that if a series is uniformly convergent in the neighbourhood of a point then it is uniformly convergent over an interval. He took a particular value a of x and assumed a quantity ρ for which $\Sigma f_\nu(x)$ was uniformly convergent for all x satisfying $|x-a|\leq \rho$:

> "I will say that the series converges uniformly in the neighbourhood of the point a. The quantity ρ then has an upper limit; let this be R, so the collection of values of x for which $|x-a| < R$ may be called--in relation to the considered series--the vicinity of x, and R its half-measure. One assumes any point one likes in this vicinity, so it is clear that the series also converges uniformly in the neighbourhood of the

> latter [point]. It follows from this, that the collection of points in whose neighbourhood the series converges uniformly is represented in the plane [Ebene] of the variable *x* by a simple surface [Fläche], but which can exist as several pieces separated from each other."

This last sentence shows that Weierstrass even had uniform convergence by intervals in mind. In footnotes he explained that a simple surface is one "which goes through no point more than once" and that uniform convergence was defined for all points of a part (or sub-region) of the "region of convergence" of $\Sigma f_{\nu}(x)$. To this latter remark he nonchalantly added a test for uniform convergence known today as "Weierstrass's M-test"!

The mixture here of symbolism and quantificationally clear natural language is typical of the period, and is significant from the points of view now under discussion. So also is Weierstrass's assumption of what we now recognise as the "Heine-Borel theorem" (whose history is briefly described in the third part of this paper), for it exemplifies other developments of the period becoming involved in the study of modes of convergence and the emergence of quantification: definitions of irrationals, conscious distinctions between upper/lower limits and least upper/greatest lower bounds, existence theorems for limits, and so on [e], [f], [w]. There was also considerable attention given to the foundations of the calculus, a branch of analysis of comparable importance to modes of convergence for the genesis of quantification: the (ever-changing) relationship between continuity and differentiability, the fundamental and mean-value theorems, differentials, and the whole range of multi-variate analysis [g]. And along with all these developments came an increasing use of symbolic language, for the distinctions now being operated were too delicate to be manipulated easily by means of natural language. It was the language of quantification, in the sense which I defined at the beginning of this part.

The passage to a fully symbolic expression of multiple limit analysis was a complex process in which all branches of the subject seem to have played a role. Examination of some principal text-books and papers suggests that symbolism became steadily more prominent and that care in explicitly using the positive values of quantities and in handling the arithmetic of inequalities correspondingly improved in standard. It would not be possible even to sketch out here these developments; indeed, such an enterprise is one of those to which these preliminary notes are intended to call attention. However, mention must be made of Peano, who worked equally in Weierstrassian analysis and in the mathematical logic of quantification and exerted by far the greatest single influence in the symbolisation of analysis. The *Formulario mathematico* which he edited in various editions (and under various titles) from 1894 to 1908 was a most important exposition of such techniques, and the journal *Revue des mathématiques* contained many research articles in these fields.

PART 2: ON THE AXIOMS OF CHOICE AND THEIR RAMIFICATIONS

I trust that the discussion above shows, albeit briefly and through exemplification, that during the Weierstrassian period a critical re-examination of the early 19th century analysis was carried out, refining its basically *single*-limit style into a form able to handle *multiple*-limit techniques with proper attention to quantification and the relationship between increments. Weierstrassian analysis received a critical re-examination of its own, and in this part I want to outline a prominent part of that revision: the axioms of choice and their ramifications.

Today we usually speak of "the axiom of choice," but one of the most interesting features of the history is that there never was only one such axiom. For during the decade following Zermelo's postulation of a selection function in 1904 [h], which crystallised into an explicit axiom of choice the occasional previous awareness of assumptions concerning infinite selections, various different axioms were suggested [i]. Of particular importance was the "multiplicative axiom" published by Russell in 1905 [j], whose need he seems to have realised independently of Zermelo (see [B18, sect. 14]).

Both axioms had arisen in connection with Cantor's *Mengenlehre:* Zermelo's in proving the well-ordering principle, and Russell's in defining the product of an infinity of cardinals. But we can associate them with the re-examination of Weierstrassian analysis, for Cantor developed his *Mengenlehre* as a consequence of his study of trigonometric series [B8] and he regarded his new theory as an extension of mathematical analysis. The axioms of choice turned out to be necessary for several parts of the *Mengenlehre* ([k, arts. 1-4]; [B18, *passim*]): the equivalence of the non-inductive and reflexive definitions of infinity; various theorems relating cardinal addition, multiplication and exponentiation; all sorts of theorems about the cardinalities of non-denumerable sets; many properties of the series of transfinite ordinals; the proof that a limit-point of a set is an accumulation point, and thus a variety of results involving closed sets, including the Bolzano-Weierstrass theorem (and hence the Heine-Borel theorem); and several decomposition theorems for the line and plane.

The axioms also arose in "orthodox" Weierstrassian analysis [k, arts. 5-9]. The transposition of universal and existential quantifiers could require an axiom if their ranges were infinite. Measure theory, in itself a major advance of that time over the treatment of integration in the Weierstrassian period [ℓ], used such axioms for theorems on infinite additivity, the construction of non-measurable sets, and elsewhere. In the theory of functions, these axioms were necessary for such a basic result as the equivalence of the Cauchy definition of continuity of a function ($(f(x+\alpha)-f(x))$ is small with α; [a, pp. 34-35]) with the Cantor-Heine definition (if $x_n \to x$ as $n \to \infty$, then $f(x_n) \to f(x)$; [m, p. 182]), never mind such sophisticated studies as analytically representable functions and Baire's classification [n].

However, the acceptance of these axioms was not unquestioned. Several theorems which used an axiom were re-proved without it, while there was controversy over whether some other theorems needed an axiom of choice. For example, only gradually did it become clear that the Bolzano-Weierstrass theorem would need such an axiom if "infinite" was defined non-inductively (as opposed to reflexively) and if the existence of a limit-point (as opposed to an accumulation point) was asserted, but would not need an axiom otherwise [k, art. 4].

A further point of controversy was whether or not the new axioms were logically equivalent to each other. For example, Russell took a few years to see that his multiplicative axiom could be cast in a form equivalent to Zermelo's axiom [B18, sect. 17]. The question was of philosophical as well as mathematical interest. All these axioms postulated the existence of a function (such as Zermelo's selection function) or a class (for example, Russell's multiplicative class), or the validity of a process (such as the construction of a maximal class) or a property (for example, the trichotomy law for cardinals); but the degrees of non-constructivity--and thus acceptability--of these axioms seemed to some mathematicians to be different [o]. For example, the French school discussed at some length (beginning in [p]) simultaneous as opposed to successive selections, and denumerable vis-à-vis non-denumerable choice, and the related questions of mathematical definition and existence. Perhaps the most substantial criticism of non-constructivity and existence at that time was Brouwer's development of intuitionistic set theory, which not only rejected axioms of choice but also the traditional use of the law of excluded middle. His papers of the early 1910s on the Cantor-Bendixson and related theorems [q] exemplify well the extent to which he broadened the issue of constructive versus non-constructive processes in set theory and analysis. Indeed, his work on set theory seems to contain valuable clues for the understanding of his intuitionistic logic.

Mention of logic brings me back again to quantification, for the word is most closely associated in the sense used in this paper with logic and was so introduced by Peirce in the

1880s [r]. Hence it would be appropriate to close this part with an example of an axiom of choice, quantification and the continuity of functions all at work together. Whitehead and Russell's *Principia mathematica* of 1910-13 contains a discussion of both the Cauchy and the Cantor-Heine definitions of the continuity of a function, both expressed in their sophisticated symbolic language [s, * 234]. Quantifiers are prominent (though not for the purpose of expressing uniformity, since mathematical analysis was not developed that far in their work). The multiplicative axiom was already embodied in various previous definitions and results. Sequences of integers and of real numbers were now fields of Fregean ancestral relations, with the continuous function serving as a mapping between them. Thus Whitehead and Russell could announce that "It will be observed that practically nothing in the theory of continuous functions requires the use of numbers." [s, vol. 2, p. 725]. As mathematical style, this was a long way from Cauchy!

PART 3: ON THE SIGNIFICANCE OF HISTORICAL SIGNIFICANCE

How very "interesting" these historical accounts may be, but do they *matter*? I often find that mathematicians see the history of their subject only as a reservoir of factual information or as a source of anecdote or biography. I shall argue in this part that a greater significance than this can be attached to historical work, greater not only for the sake of the historical record but especially for the understanding and pursuance of mathematics at all levels, including teaching and research. I shall also try to point out some of the ways in which mathematicians and historians differ in their approaches to the history of mathematics.

Let me take first the introduction of *new distinctions*. They are an important source of advance in mathematical knowledge, and have been well exemplified above: distinctions between modes of uniform and non-uniform convergence, between the two definitions of infinity, between forms of the axioms of choice, and so on. The pre-history of a new distinction is not easy to interpret historically. Suppose that since a certain time we have distinguished between A and B. How should we read the earlier literature? We may find A and no B; or B and no A; or both A and B but no explicit recognition of their relationship; or very often a C or two which are neither A nor B but something else which could be interpreted as either by means which the theory containing C cannot express.

From the point of view of mathematics, the new distinctions are a source of further work, including the reworking and rereading of previous results. But from the point of view of history, they are very likely to be a source of post-hoccery, of historical *verification* of old work in terms of some fashionable current view which in fact at that time was scarcely known, of the "correction" of earlier literature to a currently accepted form. In history we need to construct *ignorance situations* for our historical figures, that is, branches of relevant knowledge constructed between their period and ours of which they were essentially ignorant.

It is not easy to construct an ignorance situation for an historical figure, since normally he will not have said much about the problems which he could not solve and of which he might not have been aware. A recommended approach is to read later text-books, but in my opinion this method is limited in use by the degree to which the text-book writer understood the development of the mathematics which he was presenting. A much better method is to examine later research work, with especial reference to the claimed novelties of each stage of later progress. Even this approach is not infallible, for ideas and techniques were often rediscovered; but it should yield a serviceable approximation to the ignorance situation sought.

The emergence of new distinctions exemplifies the problem of the use in historical work of later ideas of *any* kind. The belief that the historian can think himself back fully into the conceptions of the period under study is illusory; even if achieved, it cannot be *proven* to have been achieved. Much more likely is that remnants at least of the work of the intermediate periods will remain, which makes it all the more essential for them to be recognised *as* later work. Sometimes these later ideas can be put to the good purpose of inspiring questions to be asked of the period which they succeeded, but they cannot be transplanted uncritically back onto the period itself. For the assumption of a later idea in an historical interpretation *necessarily* prevents the historical account of its own emergence; the pertinent historical questions cannot even be *asked*, never mind answered. For example, the emergence of quantification exemplifies the general problem of the comparative roles of symbolic and natural languages in the development of mathematical analysis, but it would be historically disastrous to insert quantifier language into any previous mathematics which could be construed as involving its use. For then it would be being taken for granted, as it were, and questions about its emergence could not be posed.

Another feature of the history of mathematics which has been exemplified above is the way in which a branch of mathematics may develop in phases, each one with its own problems and techniques which become important for a time before reducing to insignificant details or making way for fresh approaches. We saw the early stages of mathematical analysis in Cauchy's *Cours d'analyse,* which supplanted the algebraic, operator-oriented, conceptions of the calculus and of summability (not described above) dominant in the late 18th century. Then the Weierstrassian period took over for the last third of the 19th century, enriching the previous phase with multiple-limit techniques and explicit attention to quantification. Finally we saw Weierstrassian analysis itself (including Cantorian *Mengenlehre*) appraised for its unintended assumptions of axioms of choice.

One aspect of progress by phases is the change in *priority* (using the word in the sense of "importance") which theories experience, quite possibly independently of any marked change in their content. The historian must try to reconstruct the priority structure for the particular developments which he is studying. In particular, he must bear in mind that possibly it will be very different from the structure prevalent today. Mathematicians of long ago may well have studied intensively certain aspects of their work which now appear of little significance. But if they found these aspects important, then their historians must treat them as important also, and try to discover the sources of the importance then held.

By contrast and with perfect legitimacy, the mathematician will transplant his own priority structure onto any earlier work on which he is drawing, in order to enlighten his own situation as much as possible. Indeed, his interests will determine the choice of earlier work, for naturally he will discard material which today is of low priority. A danger arises, however, for priority means relevance, relevance means interconnections, and sometimes these are not of the obvious kind.

The last historical feature which I wish to emphasise is the use to which the history of mathematics may be put in mathematical education. It seems to me not only "interesting" but most valuable for students (who are *not* usually training to become professional mathematicians, after all) to see mathematics evolving as some sort of historical process. Then they can relive earlier work and create for themselves the discoveries of the past. Obviously it is not possible or desirable for *detailed* historical work to be done. But it should be feasible to present the major stages and crucial results in imitation of the historical record, with the actual historical details lightly handled. This is a process of learning which elsewhere I have called "history-satire" [t, pp. 445-450].

Consider, for example, Cauchy's false theorem, and its progress from Cauchy's proof through Abel's counter-example, Dirichlet's conditions for Fourier series, and Seidel's modification, to the introduction of modes of convergence. Learning analysis through a sequence of events like that would be much more exciting for the great majority of students than ploughing through the modern text-books on the subject, with their rigorous Chapters One and the inevitable progression of impeccable definitions and theorems thereafter. In fact, closer examination of these books often reveals less clarity of presentation than might be expected. The levels of rigour associated with each phase are often merged together. Thus, for example, the theory of limits and continuity of functions may be dressed up in topological clothing which is mostly ignored later. Again, enough set topology is provided to serve as a basis for measure theory, but the Cauchy-Riemann integral is usually developed instead. Multiple-limit analysis, whose importance Weierstrass himself emphasised in his lectures [u, p. 256], is *not* normally accorded such prominence nowadays; quantifier order will be emphasised in some definitions and proofs, and uniformity or non-uniformity introduced into various theorems, but the underlying connections and common significance are not stressed. Axioms of choice are either passed over in silence or introduced without indication of their importance beyond the printing of an asterisk against the titles of theorems in which they are used--a hangover from the days of the controversy.

A good example of the merging of phases is the name "Heine-Borel theorem," introduced by Schönflies in 1900 [B36, p. 119]. The theorem itself was proved by Borel in 1895 [v, p. 51] and was a significant result for the analysis in which the axioms of choice played a role. Heine's name was attached to it for the superficial reason that in proving in 1872 that a finite-valued continuous function is uniformly continuous over a finite interval, he partitioned the interval into a finite number of sub-intervals [m, p. 188]. This theorem is typical of the *Weierstrassian* period in being concerned with uniformity.

I have laid emphasis on the distinction between the attitudes of mathematicians and historians to the history of mathematics. In general, mathematicians make use of earlier mathematics primarily as an aid to the elucidation of old and new mathematical problems which they are trying to solve. They tend to conserve this attitude even when taking an explicitly historical interest in their subject. Thus they will interpret any given piece of earlier mathematics in terms of a current or a later version, and then evaluate it in that context. I do not say that this is "wrong"; from the standpoint of their objectives, it is probably the "right" thing to do. But they *are* wrong, I feel, to think that the later version clarifies the earlier one. It may clarify the earlier mathematics *as mathematics,* and it should clarify the later version itself; but in all likelihood it will blur the earlier mathematics *as history,* as a progress of events long ago. The mathematicians' interest in an historical period is oriented towards its post-history, whereas the historians emphasise its pre-history.

These differences of attitude reveal a paradox: that there is both interaction and autonomy in the relationship between history and mathematics. The resolution of this paradox lies at the heart of the history of mathematics. If this conference can provide some clues towards such a resolution, then it will be an important event.

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DISCUSSION

Dou opened the discussion by suggesting that it might be better to try to distinguish the concerns of the historian from those of the mathematician in terms of the tools available to the two disciplines. For example, when comparing the language used, a mathematician can use well-defined standard symbols. This provides a language which historians and philosophers cannot use and, according to Dou, the resulting discourse has a different level of precision.

Kahane then suggested that mathematicians think on two levels all the time, namely, foundations and problems, so that the distinction between the two is not sharp. Thus measure theory is a part of foundations today, but in the early 1900s it was concerned with specific problems. Birkhoff supported Kahane's distinction, asserting that problems deal with questions whose answers are unknown, whereas foundations have to do with the assumptions which must be made in order to deduce an agreed-upon conclusion. May suggested that problems can be likened to legal *questions*, while foundational issues are analogous to legal *precedents*.

Browder felt that one should take into account questions of *priorities*. These may be difficult to discern, but they can play a key role in the selection and status of problems. Thus the same question can be logically foundational, yet at an earlier time have the status of a problem whose answer people were really concerned about.

Grattan-Guinness, however, denied the importance of the distinction between problems and foundations. Instead, he proposed a third category: the mathematics of textbooks. He claimed that such mathematics is also meta-mathematics, a realm in which the mathematician is talking about mathematics as well as doing it.